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IN REGARD TO SPECIES AND SPONGES

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IN the language of systematic zoology, species are particularly "difficult to distinguish" in certain genera, in many genera of sponges, for instance. Where this is so, it is in large part due to the fact that many specimens from various regions have been reported on. In such cases we begin to be face to face with the facts (of variation) as they are, not as they are assumed to be, when the species description rests on one or two specimens or on specimens from one locality.

A species in literature starts with the description of type specimens embodying a certain combination of characters, some of which are dimensional. As more or less similar specimens from different localities are studied, more and more such character combinations become known. These are of such a kind that, in sponges at least, if we consider any particular character, for instance the nature of the cortical canal spaces in the sponge *Donatia,* or the size and shape of the little spicules known as choanosomal asters in this genus, we shall often find, perhaps always find if we look long enough, forms that are intermediate between "species" in which the character in question has been regarded as a differential, that is, as a conspicuous mark of difference. That is, in respect to any one character there are species, and indeed often genera, which intergrade. Series of this kind indicate that the characters vary, or in the production of existing races *have* varied independently of one another.

As yet the most practical way of handling such complex data is to regard the race, usually geographical, which is represented by a described type, as the nucleus of a species that is gradually to be described. Round the race represented by the type specimens, and characterized by a certain character-combination, the so-called variations are gradually plotted. In this process racial or germinal marks are, it must be confessed, often confused through the limitations of the method with more purely somatic or environmental features. After a time, species-ideas emerge that are rich in content, and usable for all kinds of biological work. Such species-ideas, however, owing to the way in which they have been built up, will undoubtedly sometimes overlap. When this becomes apparent, some rearrangement of the data (records) will be necessary, that shall be more in accord with the actual genetic relationships between the sponges and the causal nature of their differences. Such rearrangement, as we all know, has to be made from time to time. Species are combined, or distinct (hereditary) races within a species are distinguished, or marks that in the beginning were assumed to be racial are found to be age- or seasonal- or sex-marks, or the plain and simple results of a certain kind of local environment. All this is especially applicable to very inadequately known groups like the sponges, as it was equally applicable in former years to groups that are now better known. We must recognize, then, the necessity which is ever present with the classifier, of rearranging from time to time his categories.

In principle, systematists all agree with these ideas. Difference in actual practise comes in, however, when it is a question of assigning a place to a particular sponge or kind of sponge that has been described. Thus, to take an example, while very generally *Donatia ingalli* and *Donatia seychellensis*, the types of which differ noticeably in respect to the cortical anatomy, are retained as distinct species, some writers make them synonymous, viz., merge them. Of course, if in merging species the lines and magnitude of variation represented by particular races or strains (for so we may think of many literature species) are not lost sight of, no harm and often much good is done. But the merging can easily be arbitrary and artificial, as when only one or too few points of structure are considered, and the differences are slurred over, in which case distinctive characters that have been carefully recorded for this or that geographical region are lost sight of, and the process is simply the retrogressive one of "lumping" species together.

When one speaks of a species as embodying a certain character-combination, one may seem to be supporting the contention of Bateson, Lotsy and some others, that races are distinguished from one another as representing different combinations of germinal character-units, of units that have great stability; units which, moreover, are conceived of as comparatively limited in number and countable; units which are transported unchanged hither and thither through individual bodies, in the mazes of reproduction-cycles, combining, separating and recombining again to produce by concerted action what formerly they produced (phenomenon of reversion). And it is undeniable that if we assume enough such units, we can use the hypothesis and possibly to advantage.

What is obvious, however, to the student of systematics and geographical distribution, although it must be frankly confessed that we are in the case of the sponges in great need of the intensive study of critical instances, is that racial features often vary up and down, apparently in response to the environment, in such a way as is possible only to easily alterable species-plasms.

One comes to symbolize the latter after the fashion of chemistry as complexes of atoms, molecules and radicals. Change in a complex may be pictured as due to the addition of more molecules or more radicals of a certain composition, or to a diminution in the number of such units. Or we may make the picture of fewer units, and think only of radicals which lose or gain atoms or simple molecules in response to environmental (external or internal) conditions, to "stimuli" as we say in physiological language. The radical thus varies up or down, and with it the features of the resulting organism. The rays of the small asters (chiasters) of *Donatia*, for instance, become obviously spinose at the tip, or are so minutely spinose that the fact is ascertainable only when an immersion objective is used, or are not spinose at all. In such a case, whether the germinal change be essentially one of a number of units, or quality of larger units, a graded series results, a series of idioplasms, of protoplasmic compounds, corresponding to the series of organisms which the study of differences exhibited by individual animals has led us to tabulate. The terms of such series actually in existence in nature at any one time may or may not be numerous. It would, however, seem, arguing from our knowledge that so many localities stamp with minute and yet distinguishable marks the individuals of a species living in such a region, that they often are very numerous. But whether numerous or few at any one time matters not. Artificial conditions bring into existence terms of the series never seen before, precisely as in the chemist's laboratory. And what does not exist to-day may come into existence in the future. Thus in respect to any character a series exists ideally, the terms of which are marked off from one another by minute, essentially dimensional, differences. Particular, easily distinguishable, terms of the series are recorded in systematics as among the differential characteristics of species, when they occur with great constancy in a group of individuals; or as marking out a possible incipient species, when they occur only in some individuals, or when as in certain sponges they are represented only by some spicules of some individuals.

It is clear that the distinct categories of systematics or any classification, short of one that recognizes "individual-plasms," make an artificial set of frames for such boundlessly variable substances as are those that compose, let us say, the specks of protoplasm constituting the germ cells that will give rise to the organisms of the next generation.

It is no wonder, then, that although classifiers spend so much time in defining, viz., characterizing, particular species, no one can define the species-idea itself. Naturally this is so, because the species-idea is not precise, that is, it is not a definite complex of simple concepts, as was Linnæus's idea of species. The term stands only for a practical method of classifying things, which makes use, or tries to make use, of kinship as the chief guiding principle. Being of this practical character, it is in a measure apart from and implicitly contradictory of, our more precise physiological knowledge of nature, since its usage implies (of course, only on the face of things) that organisms do fall according to hereditary constitution into primary, separate groups of like significance. Nevertheless, as we all know, the employment of this practical rule, the species-idea, is a habit which helps toward the accomplishment of the great aims of biology, is indeed at present an indispensable tool in biological research.

The species-idea being of this kind, there is, of course, no definite number of species in the world, any more than there is a definite number of facts which, when found out, will constitute the perfect and closed book of a science. Reference is sometimes made to-day by biological writers to Linnæan species as if to groups of individuals that are qualitatively different from the groups which for convenience sake we put together under one name. Certainly the classifier of a group of organisms, as he extends his survey through the world, finds no counterpart in nature to an idea of a related set of individuals radically alike and radically different from all other sets.

What he finds is minute differences in the many times repeated, and superficially similar, structures of a single animal (the case of a sponge spicule, *e.g.*), differences between the individuals of a local race, and differences between groups of individuals from different localities. Whatever distinction there is between individual (if hereditary) and specific differences, is one of degree not of kind. This is a commonplace in our book of principles, and has been since Darwin's time.[1] In the practise of the systematist the race is only a form "sufficiently constant and distinct from other forms to be capable of definition." And, as Darwin says,[2] if we call it a species, it is only because we regard its peculiarities as sufficiently important for the group of individuals exhibiting them to deserve a specific name.

It is convenient, as I have said, to use chemical symbolism. But what in the germ really underlies, what is the actual material basis of the individual differences which mark off races and species? Can we ever answer this question? Is it not simply one of those that turn upon the nature of matter, which physiological analysis of our sensations tells us need not, in strict logic, exist? At any rate, the kind of problem which we apparently can solve at the present stage of our mental development is not such as deals with the structural peculiarity in the idioplasm of the germ cell, the something which, if it exists, we may be allowed to call the final material cause of a character. It is rather how to produce the character, how to start and control the series of differentiations that lead up to its appearance. Growth and differentiation of the substance of germ cells and of tissues we certainly know something about, in the sense that our knowledge of these matters is perceptual knowledge and not symbolism, except in so far as all of our knowledge of matter is, to be sure, only a set of deductions, drawn from the interaction of ourselves and something which may or may not, we do not know, be what we now think of as matter. It is not misleading, it is not obscurantism, to point out that the "architecture of the germ plasm" about which we hear a good deal is for many (opinions differ—*vide infra*) a problem of a different kind from that which deals with the classification of germ plasms (idioplasms) on a basis of their perceptible properties, and with the laws that govern changes in these properties. These latter acquisitions of knowledge, although their form of expression, viz., the grouping of the facts, must needs be altered from time to time, rest on experience which repeats itself demonstrably when we wish, whereas the ultimate structure of an idioplasm will most probably forever lie within the field of symbolism. Such deductions follow after and fit themselves to experiential knowledge. It is the generalized results of the latter in the shape of comprehensive ideas or inductions, that tell us what we should expect in a concrete case and so lead to discovery. Beside these, theories as to the ultimate structure of this or that kind of matter are comparatively barren. And such theories should, I think, be more distinctly recognized for what they are, not over-rated and presented, as sometimes happens in inferior texts, in the guise almost of finalities, at any rate, as great, simple truths.

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[1] *Cf.* "Origin of Species," sixth American edition, p. 412.

[2] "Origin," p. 425.

This feeling against the over-rating of theoretical explanation, with its concomitant effect on research in tending to develop the habit of substituting a partial conclusion drawn from one class of fact, for a thorough, comparative investigation of the phenomena, has been voiced in recent years by some of our leading thinkers.[3] So over-rated is theory, so habituated do we become to its use in school, university and public speechmaking, that, as we all know, it is deplorably common to find those who can not, try as they will, state their facts objectively. Such a schooling makes us all more or less inarticulate, except when allowed to speak in terms of our hypotheses, a condition which enormously increases the difficulty of ascertaining what has really been discovered in a field outside one's own. Goethe's verses, reading *experience* for *Leben* (although put in the mouth of Mephistopheles), are as true to-day as they were at the time of writing, when the imaginative portrayals by the biological logicians of the eighteenth century as to what goes on in development and inheritance, were so much nearer:

> Grau, theurer Freund, ist alle Theorie,
> Und grün des Lebens goldner Baum.

Finally, there is a question which all those who deal with the peculiarities and the classification of natural races and species do not answer in the same way. What place do the Mendelian genes occupy in our schema of the physiology of organic change, of the evolution of races? The gene is usually conceived as a self-propagating and stable particle of the germ plasm, which materially affects the sensible properties displayed by the organism into which a lump of such substance develops. It affects, at least in the speculations of T. H. Morgan and similar thinkers, not one but many such properties. The incorporation or loss of a gene is comparable to the incorporation or loss of a radical in an ordinary (non-living) organic compound.

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[3] *Cf.* W. K. Brooks *passim* and A. Agassiz in his presidential address before the International Zoological Congress in 1907.

In bi-sexual development corresponding genes repel one another in such a way as to fall in different germ cells. With this latter idea, the work of the systematist gives no familiarity, and I therefore pass it by, remarking only that doubtless all the facts of the inheritance of traits, even where there is great variation in the offspring, can be brought into conformity with it, if we adopt multiple-factor hypotheses and assume the presence in a germ cell of many genes which cooperate toward the same end; a whole block of such genes, perhaps, as in simple Mendelian heredity, acting as a unit and repelling a corresponding block, while under other circumstances the block is disorganized and the separate genes act independently of one another, thus leading to a greater variety in the offspring, as in cases of "blended inheritance." Or we may explain miscellaneous variation in the offspring by using some other form of accessory hypothesis, such as that which postulates the presence of primary character-producing genes and of other secondary genes which determine through their cooperation the degree in which the character is developed.

Viewed in general as a component of the idioplasm, the gene fits into our knowledge of the peculiarities that differentiate races, and a place is provided for it in the chemical representation of a "species-plasm," already referred to as a usable concept, and which dates back at least as far as Haeckel's "Generelle Morphologie," where such plasms are referred to as "organic compounds." It is, as several have pointed out, the material representative of what we more usually call a hereditary trait. The graded series, for instance, formed by related idioplasms, in the Chalinidæ, for example, in which sponges the skeletal fibers range from such as consist of many rows of spicules with but little spongin, through term after term up to fibers consisting only of spongin with no spicules, these graded series, met everywhere in the sponges, are describable, as I have said, in the terms of chemical symbolism. They are equally describable in the language of the gene theory. In the latter case, we picture genes, reserving speculation as to their ultimate relation to the chemical structure of the germ plasm, some for the production of spicules, some for the production of spongin, each kind present in considerable number and all cooperating to produce the fiber. Perhaps symbolism of this kind will prove useful in lending precision to our thinking when we come more widely in zoological work to the experimental task of analyzing through the production of new forms, the forces that bring about change in organisms.

Are genes countable or not? Sometimes writers speak as if they were. In the world of imagery, they undoubtedly are in strict logic, just as atoms and molecules are countable on the basis of certain premises in non-living substances. But the hereditary properties to which they give rise, and which, after all, as Castle in particular has emphasized, are the only facts of which we have any (perceptual) knowledge, are, as every student of nature must admit, not countable, for as soon as a new property has been discovered, it proves to be but a door that opens to the discovery of still others.

The gene-idea is, as we all know, very commonly, although by no means universally, linked up with chromosomes. And it must be said that the linkage, if it really exists, in nature, carries the idea quite out of the world of symbolism into that of cellular physiology, the "architecture of the germ plasm" becoming then a real object for experimental study and not a mere subject for logical imagery. But it will not do to forget that the facts at least permit us, if they do not force us, as some maintain, to look on the visible material particles (chromatin masses), with which "characters" are associated (and the progress of genetic and cytological researches seems unquestionably to show that such exist) not as determinants, but as differentiations—as indeed the first conspicuous differentiations that are made by the idioplasm in the course of the chains of events which lead to the appearance of particular characters. Viewed in this light, chromosomes lose that air of finality with which the Weismannian philosophy has invested them, but retain a very solid interest. Genes, however, recede into the invisible.

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The records that make up biology would be chaos without systematics. Doubtless all will admit that much. Again, the interest in discovering new forms of life, new races and species, which is an essential part of systematics, is a basic factor in revealing what Arthur Thomson calls the "web of life," the complex of friendly and disastrous influences which species exerts on species in the matter of living. What is not so generally realized is that the classifying spirit and method, which works under comparatively simple conditions in systematics, and finds therefore in this field a chance to grow, is something that is needed in every branch of biology, if we do not wish to remain content with very provisional and partial conclusions concerning the causes of phenomena, conclusions which sometimes succeed one another with a rapidity that dazzles and confuses all but the few engaged in their manufacture. This is only saying that science is nothing unless comparative. Systematics has, therefore, I believe, a great educational value.

But what direct significance have the results of systematics for the study of the laws of change in the organic world? Systematics reveals, at least in sponges, that characters are independently subject to variations such that in respect to any one of them, individuals and races occur which form close series between far distant extremes. Such series are found everywhere in the group. They are doubtless in a measure phylogenetic series, in which the terms bear to one another the relation of ancestral species and descendant. But the kaleidoscopic combinations of characters displayed in many genera and families indicate that this is not always the real relation. In these instances, it would seem rather that the terms of the series represent only different degrees in the response to the environmental stimuli, which related idioplasms have carried out independently of one another. Systematics thus gives us all the time hints, in some cases misleading perhaps, as to what *species-plasms can do,* and as to the conditions under which such and such a thing is done. It is a good guide to experiment, without which the latter is apt to run off into vagaries.